THURSDAY, JUNE 17, 1875

CROLL'S "CLIMATE AND TIME"

Climate and Time in their Geological Relations; a theory of Secular Changes of the Earth's Climate. By James Croll, of H.M. Geological Survey of Scotland. (London: Daldy, Isbister, and Co., 1875.)

MR. CROLL is well known as an original thinker of considerable power, who has turned his attention to the physics of geology, and has produced a series of remarkable papers on questions of the highest interest in that subject. His views are opposed in many respects to those accepted by other influential thinkers, and have given rise to a considerable amount of controversy. Hitherto they have been scattered in papers to various periodicals, and it has been difficult to obtain a consecutive view of them. The work which is now issued, while not an actual reprint of previous papers, is a complete exposition of their contents, or at least of that part of their contents that Mr. Croll is prepared to stand by, some of the arguments that occur in his papers being omitted in his book. We are now therefore able to judge fairly what truth there is in Mr. Croll's ideas, and to compare them with those of his opponents. Even were all his ideas untenable, we should still have to thank him for his vigorous discussion of these interesting questions, but there can be no doubt that in many instances he proves his point.

Mr. Croll does not possess the happy faculty which some authors have of carrying his readers with him: on the contrary, his style is so controversial, that to agree with him is to have the feeling of being vanquished, and the reader is throughout set on his metal to find out some flaw in the argument. This, as in most cases of controversy, it would not be difficult to do; but we must confine ourselves to the discussion of his main results.

One peculiarity of Mr. Croll's arguments must here be noticed. After having assumed certain figures and arrived by their means at definite results, he proceeds to show that these figures are unreliable, and then to state that their unreliableness will not affect his results; or else, in order to bring his results more into accordance with received opinions and probable facts, he generously halves them or diminishes them still more, apparently unaware that had his arguments been correct and his first results the true ones, he would have proved too much and refuted himself. Examples of this peculiarity will be seen in the sequel.

The first question discussed is the heating influence of the Gulf Stream. To estimate this Mr. Croll uses the method of heat units, and prides himself on doing so. The method is an undeniable one, and is perhaps the only one by which the influence of the high specific heat of water can be made manifest. Mr. Croll compares the number of foot-pounds conveyed by the Gulf Stream into temperate regions with the number due to the heat of the sun shining directly on those regions. The relative value of these depends on the absolute value of each. The volume, velocity, and temperature of the Gulf Stream have been very variously estimated; and as to the sun's heat, when we remember how much the diathermancy of the air depends on its condition, we may not be able to accept with such confidence as Mr. Croll, the estimates of Pouillet; yet with every possible allowance, when the influence of a vast body of heated water is calculated, it will undoubtedly be much greater than would have been previously supposed, and actually amounts to a very considerable fraction, say $\frac{1}{16}$ of the whole of the sun's direct heat on the North Atlantic. Dr. Carpenter* brings objections against this method which render, in his opinion, the "figures" "utterly valueless." The first of these is that Mr. Croll does not give a correct account of the difference in temperature between the northern and southern hemispheres in assigning it to the transport of heated water by ocean currents; but it is obvious that the question as to where the Gulf Stream obtains its heat is entirely distinct from that as to its actual amount. The second objection, that since the temperature of the ocean is seldom more than 82°—86°, while the "direct heat of radiation" may amount to 215°; and therefore that "the heat lost by evaporation from the sea must be far greater than that lost by radiation from the land," is just one that shows the *value* of Mr. Croll's method. For, when treated in this way, the above figures show that the sea contains *more heat units* in its heated surface stratum than the layer of land that is influenced by the variations of surface-temperature, and that therefore the water at the equator is, as Mr. Croll states, the best adapted for retaining the heat of the sun, which is in reality no more than an elementary result of its high specific heat. Mr. Croll considers that the influence of the Gulf Stream is indirect, being manifested by the warming of the S.W. winds; and to the extent that he proves that the Gulf Stream raises the general temperature of the Atlantic he cannot be wrong. Were he to confine himself to the statement that the Gulf Stream and other ocean currents have a very sensible influence on the climate of the temperate regions, his position would appear to be impregnable against any who should represent its thermal effect "as very insignificant;" but when he adds that "ocean currents are the great agents employed" (to the exclusion of others) "in the distribution of heat over the globe," and estimates that the Gulf Stream alone raises the mean temperature of London 40°, he stands upon less certain ground. For these results depend on the following arguments:—(1) There is no ocean circulation but that by sensible currents; (2) The internal heat of the earth has no influence on climate; (3) The temperature of space is − 239° F.; and (4), the Gulf Stream supplies $\frac{1}{5}$ as much heat to the Atlantic as the direct rays of the sun. Of these arguments we will below discuss the first at length. The second is founded on a statement of Sir Wm. Thomson's, that an increase of temperature as great as 2° F. per foot in descending into the earth would not have an influence of more than 1° on the climate of the surface. This, however, means 1° over the present mean temperature, and in no way disproves that the internal heat of the earth does nothing in raising the temperature of its surface over that of space, an effect which it most certainly would have in a large degree. The third argument, as to the temperature of space, is therefore nothing to the point, and is moreover, as Mr. Croll himself admits, totally unreliable. We do not know the temperature *from* which the sun raises the earth, except that it is greater than that of space. The fourth argument, of course, is nothing without the first three, and the fraction $\frac{1}{5}$ we have seen may be much too large. We are not, then, in a position to estimate accurately the thermal effect of the Gulf Stream and other ocean currents; but we may consider it proved, as is indeed generally acknowledged, that they have a very sensible influence, and, as we shall see, bear a great part in the general circulation of the ocean water.

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* Proceedings of the Royal Society, June 13, 1872.

We must now examine how far Mr. Croll establishes his position that a general oceanic circulation is impossible under the influence of temperature and gravitation alone. Dr. Carpenter has already given (Proc. of Roy. Geog. Soc., vol. xviii.) his reasons for his belief in the adequacy of these influences, and his replies to Mr. Croll's objections, some of which are discussed in this volume in no less than four chapters. Although it may be familiar to most of our readers, it will be well to give here an outline of Dr. Carpenter's "doctrine."

The chilling of the salt water in both polar regions renders it heavier and causes it to sink, its place being supplied from the warmer water of lower latitudes, which is itself supplied by the motion of the water from the two poles towards the equator along the lower portion of the ocean; and these two masses meeting each other near the equator, well up there, and bring the colder water nearer the surface, while the heating of the surface water in these regions keeps up the difference between the specific gravities of the water supplied to and leaving the polar regions, on which the whole depends. These appear to be Dr. Carpenter's latest views (Proc. Roy. Geog. Soc., vol. xviii., June 1874), though Mr. Croll's objections seem, in some part at least, to be aimed at details that do not affect the fundamental conception. This is distinguished as a *vertical* circulation, because the first origination of the motion is supposed to be in the *descent* of the polar waters. Mr. Croll assents to the facts, but ascribes the circulation to the initiation of the winds, and denies that there is any circulation beyond that produced by currents. We know that currents exist on the surface, and it is generally agreed that they owe their origin, in great part at least, to the system of prevailing winds, and even on Dr. Carpenter's theory they must, so far as they tend polewards, decrease by so much the general circulation of the upper ocean; but the known or assumed under-currents are much more local, and the depression of temperature at great depths is too general to allow us to conceive that the return should be made by circumscribed currents.

In discussing the question whether the polar cold is sufficient to cause circulation, Mr. Croll first objects that the sea of the tropics is salter, and therefore denser, than that of the poles, and that this would counteract the effect of the cold. There is in reality but little force in this objection as against Dr. Carpenter's theory. The excess of temperature and of salinity counteract each other in the surface layers of the tropics, and prevent them sinking or rising; but as they have a nearly horizontal motion, according to the theory, the objection is nothing, the lower layers which alone have an upward vertical motion deriving it from a *vis-à-tergo;* and with regard to the polar area the lower layers cannot be more salt than the upper, from whence they come, according to the theory, and any *freezing* on the surface must leave the remaining water on the contrary salter.

The next objection of Mr. Croll is far more formidable, though it shows that some of the *proofs* adduced are untenable, rather than the theory itself. The drifting of icebergs from Newfoundland across the Gulf Stream, and of the Atlantic cable buoy which travelled six hundred nautical miles in seventy-six days, adduced by Dr. Carpenter as proving the southward motion of the deeper layers, proves too much according to Mr. Croll, as it proves the existence of a sensible *current*, which Dr. Carpenter admits cannot be formed by differences of gravity. This may be true, and prove that other causes operate in the motion of large masses of water; but while destroying one argument in favour of, it proves nothing in opposition to, the doctrine of general oceanic circulation. This class of objections, however, are far more forcible than theoretical ones; and the list of phenomena that may be accounted for on either theory, and of those that cannot well be accounted for on the gravitation theory, *e.g.* the southward currents of Davis Straits and the east coast of Greenland, shows that neither theory *alone* will satisfy all the conditions to be fulfilled. Mr. Croll, however, gives no satisfactory account of the greater cold of the lower strata of the South Atlantic, nor of the surging up of cold currents on eastern shores, nor of the cold water coming nearest the surface under the equator; nor does his theory give that beautiful account of the maintenance of life in the deep sea which is so dependent on the change of the water.

But Mr. Croll asserts that the gravitation theory is physically faulty, and maintains the assertion in this volume against Dr. Carpenter's last reply. In several of his arguments it is impossible not to agree with him. In examining them we will follow the order he takes. He first shows that heat at the surface, as in the equatorial regions, cannot produce circulation. But this, though essential to Lieut. Maury's theory, has not been asserted by Dr. Carpenter, who, on the contrary, states that any effect due to the heating at the equator may be practically disregarded; and why? because the heat is here applied at the top instead of at the bottom, as it should be to produce convection; but an application of cold to the top would be equivalent to heat at the bottom, and this cold is obtained in the polar area; consequently Dr. Carpenter regards polar cold as the *primum mobile.* Mr. Croll objects to this that it is the *difference* of temperature only we have to do with, and this may be said to depend on either, and accuses Dr. Carpenter with confusion of ideas; but this is scarcely fair after arguing against the heat being available to produce motion because applied at the top, showing that he perceived that not the difference *only*, but *where* the lower temperature is found is of consequence. Dr. Carpenter would say the temperature at the poles is *below* the average, no matter how that average is obtained; which is a very different thing from saying the equatorial temperature is *above* the average—since in the first case the average might be obtained, as far as the theory is concerned, by a nearly *uniform* temperature elsewhere.

The next important question raised with respect to this theory is the amount of force which is exerted to put the water in motion. This Mr. Croll shows to depend entirely and only on the amount of slope of the water-surface from the equator to the pole, and not at all upon the amount of fall from the surface in the polar regions, to the lowest depth at which the water of maximum density is found beneath the equator. Dr. Carpenter says that this "would seem irreconcilable with the simplest principles of physics," a statement easier to make than to prove. For, as Mr. Croll shows plainly, the work done *by* gravity in the descent, is done *against* gravity in the ascent at the equator, and the two counteract each other, except only the extra amount of gravity which is called into action by the shrinkage of the polar column from what would have been its size under the *average* amount of solar heat, and which alone can have any *continuous* effect. The solar heat is a constantly supplied moving force which is used indirectly in the ocean circulation, and any further amount of gravity made use of in the circuit would involve the idea of perpetual motion. Connected with this is Dr. Carpenter's assertion that there is *no* difference of level between the equatorial and polar seas. Since, however, this is the *only* proximate cause for the ocean circulation, its denial would seem to cut the ground from beneath his feet. It will be found, however, that though he denies it in one place he asserts it in another, and his theory essentially depends on it. It is true that water *tends* to find its level when disturbed, as it is by the action of polar cold, which tends to alter its level; but it is just this *tendency* that causes the circulation. If one of the forces were to be powerful enough to have its own way entirely, no motion could occur; *i.e.* if the water were too viscous, a greater permanent change of level would arise; if it were *perfectly* fluid, the equilibrium would be brought about *instantaneously* and no visible motion would be perceived. We must be content, then, with the fall of level from equator to pole to produce the circulation: is it sufficient? This depends entirely on the viscosity of water. Mr. Croll bases his argument on the experiments of Dubuat, who showed that water would not descend a slope of 1 in 1,000,000, which is much greater than the slope under discussion, and hence the fall of level is too small to cause any circulation. He replies to Dr. Carpenter's objection, that these experiments had reference to water running over solids and not over itself, by saying that one layer of molecules alone would be in contact with the solid and the rest with the water surface only. The reply is plainly beside the mark, as Mr. Croll should have seen by reading Dr. Carpenter's statement following his objection, that the difference between a fluid restoring its own equilibrium, and having a sensible motion over solid surfaces, was well known in practice to Mr. Hawksley and other hydraulic engineers. But in reality no chamber experiments can determine such a point satisfactorily; and besides this, it seems to us that an important point has been overlooked by Mr. Croll. No doubt it would be hard for a single pound of water to perform its whole circuit against all opposing frictions under the impulse of the force due to so small an amount of slope; but if large masses of water move together, the moving force would be proportional to the mass, but the friction to be overcome would be simply that of the perimeter of the tube of flow, and it is an essential part of the theory of ocean circulation that the moving water is of immense mass. This friction would not increase, like statical friction, with the mass, since the pressure would be the same at the same depths, and it is also more of the nature of shearing force than friction, and therefore nearly a constant quantity.

It does not appear, then, that anything that has been said by Mr. Croll disproves this theory of a general oceanic circulation, though he may have successfully attacked it in certain respects. Nevertheless we agree with him that "if a vertical motion follows as a necessary consequence from a transference of water from the equator to the poles by gravity, it follows equally as a necessary consequence from the same transference by the winds; so that one is not at liberty to advocate a vertical circulation in the one case and to deny it in the other."

This was the opinion also of Herschel in his letter to Dr. Carpenter, that "henceforward the question of ocean currents will have to be considered under a twofold point of view." It would take too long to discuss the other points in which Mr. Croll enters into controversy with respect to various currents, such as the Gibraltar or the Baltic, and we must reserve for another notice the interesting points connected with past time with which the latter part of the book is occupied. J. F. B.

(*To be continued.*)

HILDEBRANDSSON ON UPPER ATMOSPHERIC CURRENTS

Essai sur les Courants supérieurs de l'Atmosphère, dans leur Relation aux Lignes Isobarométriques. Par H. Hildebrand Hildebrandsson. (Upsal, 1875.)

CLEMENT LEY, in "The Laws of the Winds prevailing in Western Europe," expresses his opinion, based on observations made near Hereford on the movements of the cirrus cloud, that in general the upper currents of the atmosphere flow away from the regions of low atmospheric pressure, and converge upon regions of high pressure. This being a point of the utmost importance with reference to the general circulation of the atmosphere, M. Hildebrandsson, in December 1873, organised a systematic observation of the cirrus cloud in Sweden. Twenty of the Swedish meteorological observers engaged in the work of observation, the network of stations extending over nearly 11° of latitude, from Tomarp in the south to Qvickjock in the north. The above essay discusses these observations with great ability.

The question of the relation of the motions of the cirrus cloud to areas of high and low pressure is cleverly handled in the essay, and the method of discussion, illustrated by thirty-three charts, may be referred to as a satisfactory and exhaustive treatment of the data of cirrus observation, which are restricted only to one country. Charts I. to VIII. represent well-selected instances of storms advancing on Sweden from westward; Charts IX. to XVI. represent Sweden in the rear of storms; and Charts XVII. to XXIV. represent areas of high pressure in various directions, S., E., &c., from Sweden. Comparing the direction of the upper currents with these areas of high and low pressure, it is shown that quite near the centre of the depression area of storms the upper currents blow in directions nearly parallel to the isobars and to the winds on the surface of the earth, but that in